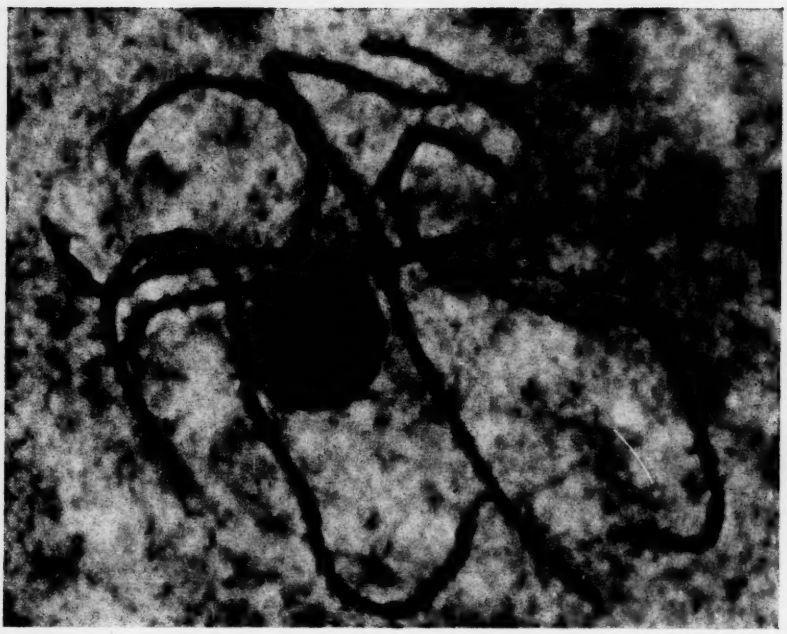
The Journal of HEREDITY



CYTOGENETIC CYNOSURE

(SEE PAGE 252)

Homage to Nikolai Ivanovitch Vavilov: 1887-1942

Tomato Flesh Color

Feeblemindedness and Rh

Chromosomes of Chironomus

The "Black Baby" ——— Myth or Reality?

VOLUME 38 AUGUST, 1947 NUMBER 8

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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The

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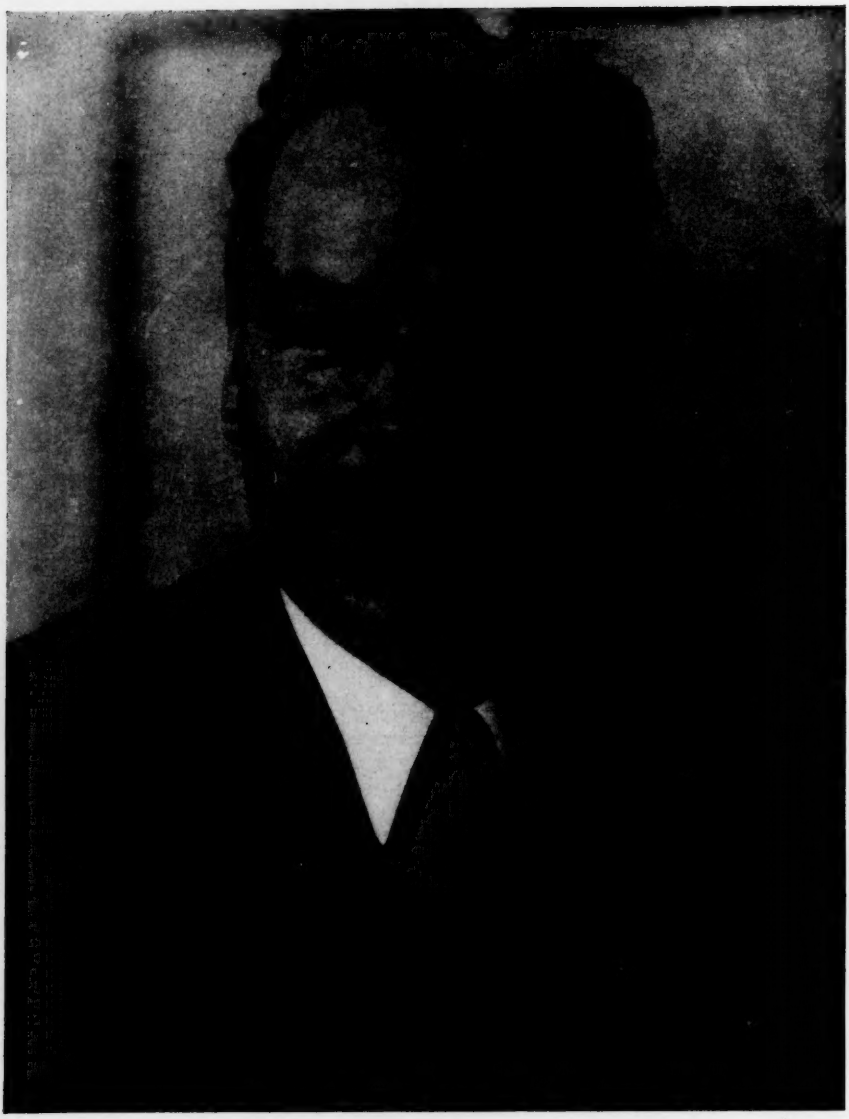

N. I. VAVILOV
1887—1942

This photograph of Dr. Vavilov was made at the time of the Sixth International Genetics Congress, at Ithaca in 1932, when he was at the height of his activity in assembling plants from all over the world for use in speeding the production of improved varieties for the USSR.

N. I. VAVILOV, A MARTYR OF GENETICS

1887—1942

TH. DOBZHANSKY
Columbia University

BIOLOGISTS everywhere have been concerned for almost a decade about the fate of N. I. Vavilov, the most eminent of Russian geneticists. The magnitude of the tragedy of this man has, however, become known only recently. It is now clear that genetics owes homage to his memory not only because his contributions as an original investigator have been great and his work as organizer and leader of research has been outstanding. He has suffered martyrdom for genetics. In our time, being a scientist is not usually considered a dangerous occupation, but rather one of the ways to relative security and to the little amenities of middle class existence. Not so with Vavilov. He ignored personal comforts and gave himself up entirely to his work. In his last years he had to suffer the anguish of seeing the results of his efforts being pulled down by incompetents. He met death a prisoner on the bleak and forbidding shores of Eastern Siberia.

Nikolai Ivanovich Vavilov was born in 1887, a son of a very wealthy merchant. He came from the curious class of "illustrious merchantry" of old Russia, which for centuries tended to become a closed caste, expert at profit taking but not otherwise noted for cultural achievement. Yet, in the late nineteenth and early twentieth centuries this class suddenly produced a large crop of leaders in almost all fields of intellectual endeavor. Among these leaders were Nikolai Vavilov, the biologist, and his brother Sergei, a physicist and currently president of the Academy of Sciences of USSR. The young Vavilovs had the opportunity to receive the best education which money could purchase. Nikolai chose as his specialty biology and agriculture. He graduated from the Agricultural Academy at Petrovsko-Razumovskoe (near Moscow), and soon went to continue his education and research at Cambridge University in England. There he became a student and a close friend of William Bateson, and one of the group of pioneer geneticists of that day. In 1913 and 1914 he worked at the John Innes Horticultural Institute founded and organized by Bateson.

Even before his sojourn in England, Vavilov had begun to study the immunity of cereals to fungus diseases, and published several papers on this theoretically interesting and practically important subject (1913). He then proceeded to investigate the genetic basis of the immunity, and found it adequately describable in fairly simple Mendelian terms. After his return to Russia at the start of the first world war, Vavilov commenced his great work on the origin of cultivated plants. This monumental study, unquestionably Vavilov's most important research contribution, was published in 1926, in a book which remains a classic, namely *The Centers of Origin of Cultivated Plants*. Here Vavilov gave a synthesis of the mass of information accumulated in the botanical literature since de Candolle, and developed his theory of several principal centers of origin and of concentration of genetic diversity in cultivated plant species. This work brought him worldwide recognition and acclaim. His other important theory, that of "The Law of Homologous Series in Variation" (1920, 1922, 1935), is an empirical generalization which states that related biological species tend to parallel each other in hereditary variability.

Organization of Soviet Genetics

The Russian revolution opened an unlimited field of applicatiton for Vavilov's energies. After an interlude as a professor at the University of Saratov

(1917-1920), he was placed in charge of the Bureau of Applied Botany in Petrograd (1920). Within a few years, he developed this rather inconsequential bureau of the old Ministry of Agriculture into one of the largest and most active research institutions devoted to agricultural sciences anywhere in the world. He not only did a prodigious amount of organizing work at home but also traveled all over Europe and the United States to establish contacts with scientists in other countries. Most important, he brought back with him a whole library of scientific literature, the supply of which had been cut off for several years by the war and, afterward, by the blockade of the revolution. Active biologists from all over Russia flocked to the library of Vavilov's new Institute to become acquainted with the advances of world science. Not a few of these visitors ended by joining Vavilov's rapidly growing staff of collaborators. Others undertook new types of research integrated with the work of the Vavilov group. Still others carried away with them some of the inspiration and enthusiasm which prevailed in Vavilov's entourage. In a few years, the All-Union Institute of Applied Botany and New Crops, as the Bureau was now called, became the center of a federation of agricultural research institutes distributed all over the USSR, from the Polar Circle to the subtropics of Caucasus and Turkestan. The combined staffs of these institutes in 1934 amounted to about 20,000 persons.

Despite the enormous weight of his administrative responsibilities, Vavilov managed to remain the *de facto* scientific and research leader, as well as the president, of this unprecedented research organization, which underwent one more change of name, to become known finally as Lenin's All-Union Academy of Agricultural Sciences. His seemingly boundless energy and vitality sufficed both for administrative duties and for creative research. Recognition came to him from all sides. He was made a member of the Soviet's Central Executive Committee (corresponding approximately to the American Senate), and was one of the very few non-communist members of that body. In 1929, he was elected member of the Academy of Sciences of USSR, and in 1931 president of the All-Union Geographical Society and director of the Genetics Institute of the Academy.

World Traveler

The practical problems of the improvement of agriculture in USSR were approached by Vavilov with his characteristically sweeping breadth and vision. He recognized that new and improved varieties of economic plants can be created only by combining together valuable genes. Such genes can be found scattered in the existing varieties, thoroughbred as well as primitive ones, all over the world and, especially, at the "Centers of Origin." A scientific breeder, he realized, must first of all take stock of the available resources, of the genetic raw materals, which will be useful to him in his work. Such a global inventory is completely out of the question for an individual scientist, or for a small group of scientists. But an Institute such as he had built, backed by the resources of a socialist state, could, so thought Vavilov, undertake this historic assignment. Hence, Vavilov and his colleagues journeyed and collected scientific materials in approximately sixty countries in all parts of the world. The scientific and practical value of the collections assembled in the institutions administered by Vavilov was inestimable.

Vavilov himself was probably the most widely traveled biologist of our day. He visited the United States and Western Europe several times, and was personally well known to, and liked by, most of his genetical and botanical contemporaries. But he was also an explorer not afraid to penetrate, with a very modest, not to say meager, financial support, places remote from well-trodden paths. As early as 1916, he had explored Persia, Turkomania and Bokhara. In 1920-1923 he visited various parts of Middle

Asia, including Tadjikstan and Pamir. In 1924 he covered a long and arduous itinerary in Afganistan. In 1925 he went to Khiva. In 1927 he studied the Mediterranean region from Portugal and Morocco to Syria and Trans-Jordan, and made an expedition to Ethiopia and Somaliland. In 1929 he visited Chinese Turkestan (Tarim Depression) and Songaria. Finally, in 1930 and 1932-1933 he traveled and collected extensively in Mexico and in Central and South America. The country which he was unable to visit despite his eagerness to do so was India, because its government steadfastly refused to admit him. Professor H. J. Muller informs me that, by a tragic irony, Vavilov was finally invited to visit India in the fall or winter of 1937. But when this opportunity came at last, he was refused permission to go by his own government, that of the USSR. The results of his activity can be gleaned from the fact that more than 25,000 samples of wheats alone were collected and grown in experimental plantings in various parts of USSR.

Devotion to Science

Vavilov was first and foremost a man of action. His energy, forcefulness, and working ability were marvelous. He was actually able to get along on between four and six hours of sleep per day, and appeared to neither need nor desire any rest or recreation. No wonder that his collaborators considered it as something less than a privilege to travel or to live in his company for many days in succession. Comforts and conveniences counted for little with Vavilov. In the nineteen-twenties, while he was one of the most influential scientists in the USSR, he lived in his office at the Institute of Applied Botany, an oldish leather-covered davenport serving as his bed. His meals were prepared by a janitor's wife, who did not excel in the art of cooking. When he married his scientific collaborator, E. I. Barulina, the new Mrs. Vavilov discovered that the household expenses for the first few months had to be met chiefly from her own salary. His much larger salary had been pledged for assistance to all manner of people, some of them only slightly known to him.

Despite being forced to lead the life of a busy executive, Vavilov neither abandoned his own creative research nor let it be performed in his name by assistants. More than that, he found time to read the current scientific literature, and was always fully conversant with the recent advances in genetics and in agricultural research, which is something very few scientists holding executive positions manage to do. He always enjoyed discussing current research problems with other scientists, even with beginners. But to have the privilege of such a discussion, one was often given an appointment at some altogether unorthodox hour. Vavilov did not have one iota of the self-conscious eccentricity of an eminent man, and his manner of address was equally direct, cheerful, and sincere with his equals and with his juniors.

To Vavilov, all worth-while scientific problems seemed to have bearing on the welfare of the whole world, and, hence, the whole world had to be called on to contribute toward their solution. Almost every publication that came from his pen attests this truly cosmopolitan spirit of its author. He was irresistibly fascinated by grand scales and by the world-wide implications of his ideas. In the words of one of his friends, a fundamental trait of Vavilov's personality was that he disliked quantities of less than a million. And, yet, Vavilov was an ardent Russian patriot. Outside of Russia, he was regarded by some as a communist, which he was not. But he did wholeheartedly accept the revolution, because he believed that it opened broader possibilities for the development of the land and of the people of Russia that would have been otherwise. In October 1930, during a trip to the Sequoia National Park in the company of this writer (and with nobody else present), he said with much emphasis and conviction that, in his opinion, the opportunities for serving mankind which existed in the USSR were so great and so inspiring that for

their sake one must learn to overlook the cruelties of the regime. He asserted that nowhere else in the world was the work of scientists appreciated more than in the USSR.

The Lysenko Controversy

A Congress of Genetics, Plant- and Animal Breeding, attended by about 1400 members, assembled in Leningrad in 1929 under the presidency of Vavilov. Among 348 papers read at this Congress, a fairly interesting but in no way revolutionary study on the physiology of cereals had as its junior author one T. D. Lysenko. A few years later the name of Lysenko was destined to become familiar not only to biologists but to newspaper readers throughout the USSR. He was hailed as the discoverer of vernalization, a process whereby winter wheat can be influenced to produce a crop if sown in the spring. The phenomenon of vernalization had been discovered in the United States years before Lysenko gave it a name; but Lysenko certainly proved himself a master of the art of modern publicity. He claimed, or it was claimed for him, that vernalization inaugurated a new era in Soviet agriculture, permitting, among other things, the culture of cereal crops much farther north than was formerly possible. The vernalization bandwagon was highly popular some ten to fifteen years ago but it is perhaps significant that little has been heard about practical applications of vernalization in the USSR or anywhere else in recent years.

Vavilov welcomed Lysenko's debut; although his published praises of Lysenko sound a bit hollow, he urged facilities for testing Lysenko's ideas. Lysenko was, however, interested in much bigger stakes. Sometime in the early nineteen-thirties, Lysenko formed an alliance with I. I. Present. Present was neither a biologist nor an agriculturist, but a specialist in the philosophy of dialectical materialism; he was also a highly effective polemical speaker and writer, and a possessor of a cultural refinement conspicuously lacking in Lysenko. In 1935 and 1936, Lysenko, Present, and their followers struck. In a stream of magazine and newspaper articles and speeches, they declared genetics to be inconsistent with dialetical materialism and with Darwinism as they construed the latter, and to be, in fact, tainted with fascism and with Nazi race theories. Furthermore, they contended, Vavilov's basing the work of plant and animal improvement on genetic principles had caused inexcusable delays in the successful outcome of this improvement work. Vastly more spectacular, and anyway vastly more rapid, practical attainments would come if only Vavilov's mismanagement and the suspect "Mendelian-Morganian" genetics were supplanted by Lysenko's patriotic leadership and the incorruptibly dialectico-Darwinistic approach.

In 1935 it was a deadly serious matter to be accused of having slowed down the development of agricultural production in USSR. To consider these charges a new Congress on genetics and agriculture was convened in Moscow in 1936, presided over by A. I. Muralov, a high governmental dignitary. The published transactions of this Congress make painful reading. There were Lysenko and Present with a well organized group of followers, pleading, cajoling, and threatening. Several geneticists, among them H. J. Muller, the visiting American, vainly tried to stem the tide against genetics. The least inspiring sight was that of some competent scientists who attempted to sit on the fence or who made unctuous speeches praising both factions. Vavilov himself made two speeches in defense of modern genetics and agricultural science. To judge from the published texts, those speeches lacked Vavilov's customary forcefulness and optimism, as though he felt that the issue had already been decided against him. And indeed, the 1936 genetics Congress turned on the whole against Vavilov, just as the one held in 1929 gave him his greatest triumph.

This rejection of sound scientific principles of established practical value, in favor of a witchcraft supported

only by artful propaganda and by big promises, seems utterly incomprehensible. The subversion and demolition of the work on plant improvement organized so successfully and on such a vast scale by Vavilov undoubtedly caused a setback in the development of agriculture in the USSR. Since, even with the energy of another Vavilov, such an organization could not be restored overnight, this blunder has harmed, and will continue to hamstring this development for some time to come.

No matter what else may be said about their intentions, those assembled at the 1936 Genetics Congress, including a majority of Lysenko's followers, doubtless sincerely desired the betterment rather than the deterioration of Soviet agriculture. This paradox can be understood only in connection with certain peculiar features of the development of biology, and particularly of evolutionary thought, in the USSR.

Genetics and Marxism

Some of these features have recently been analyzed by Hudson and Richens and by Beale.* Without going into detail, it may be stated that Lysenko and Present have exploited for their own ends an old antagonism toward genetics which had existed amongst some biologists but also to a greater extent among the general reading public in USSR. This antagonism arose because of an unfortunate misunderstanding of the meaning and implications of genetics by Timiriazev, a highly respected intellectual leader, and by Michurin, a successful horticulturist. Timiriazev and Michurin regarded the early work of genetics, especially that of Bateson's school, as subversive to Darwin's evolution theory and, in fact, a product of "clerical reaction" against evolutionary biology. The opinions of Timiriazev and Michurin carried, and still carry, great weight not only because of their scientific authority but also because of the political eminence they achieved. Vavilov and the other geneticists in the USSR were, of course, aware of this antagonism, but they hoped that it would be dissipated as a better understanding penetrated the public mind. Lysenko and Present fanned the antagonism to an inferno of contention in their attempt to unseat Vavilov and to grasp his place of leadership for themselves. Accusations of neglect of practical work were combined with indictments for heresy against approved philosophic principles.

In any scientific community individuals are not unknown who try to build their reputations by criticizing the work of others rather than by producing original ideas or work of their own. After the Moscow Genetics meeting in 1936, there was an open season for fault-finding regarding Vavilov's research and organizing activity. Dozens of hitherto unknown authors suddenly discovered that Vavilov's theories of the Centers of Origin and of homologous series in variation were totally unfounded. Worse than that, those theories had led Vavilov to dissipate his efforts by sending expeditions to many foreign lands, instead of confining himself to studies on local varieties in the USSR, which would have resulted in greater practical achievements. Vavilov was also accused of causing plant breeders to rely on the method of sexual hybridization which is practiced everywhere in the world, instead of the method of "vegetative hybrids" proposed by Michurin and Lysenko. Vavilov was told patronizingly by a certain I. M. Poliakov that "it is not necessary for you to bow slavishly before foreign science." And indeed, some breeders in the USSR promptly switched from sexual to "vegetative" hybridization. But Vavilov's worst sin, which negated all his research and practical activities, was his backsliding from canonical Darwinism (as handed down from Darwin through Timiriazev, Michurin, and Lysenko) into the Mendelian-Morganian heresy. This set-

*P. S. Hudson and R. H. Richens, "The New Genetics in the Soviet Union," Cambridge 1946; G. H. Beale, "Timiriazev, Founder of Soviet Genetics," *Nature*, vol. 159, 1947.

ting of Darwin as an incontrovertible authority in opposition to genetics, is one of the weirdest chapters in the unbelievable story of Lysenko's rise to power. The fact that genetics is the foundation of modern Darwinism proved to be no obstacle in the Lysenko-Present campaign.

Downfall and Exile

In August 1939, the Seventh International Congress of Genetics was held at Edinburgh, Scotland, and Vavilov was invited to become its President, thus receiving the highest honor which the consensus of opinion of the world's geneticists can bestow. He accepted the invitation. But less than a month before the Congress was to open, came a letter, signed by Vavilov, which stated that "Soviet geneticists and plant and animal breeders do not consider possible to take part in the Congress," because the latter was to be held outside the USSR. Few if any members of the Congress had any illusions as to whether Vavilov was a free agent when signing this letter. Matters were moving rapidly toward a denouement. In October 1939, a "Conference on Genetics and Selection" was held in Moscow, at which the problems thrashed out at the 1936 meeting were gone over again, with Lysenko, Present, and others greatly expanding their claims as to the theoretical soundness and practical efficacy of their "Darwinism." Vavilov, interrupted and heckled from the floor, delivered what was probably the weakest speech in his life, his attitude being almost entirely defensive, although he courageously reasserted the soundness of the basic principles of genetics. He evidently was already a broken man.

After the 1939 Genetics Conference, a shroud of silence envelops Vavilov. The closing chapter can be reconstructed only from unofficial, fragmentary, but apparently reliable information. Vavilov was arrested, probably in 1940. Part of the time during the winter 1941-1942, he was a prisoner in a concentration camp at Saratov (ironically, it was at the University of Saratov that he held his first post under the new revolutionary regime), and whence he was transported to Siberia. His destination was Magadan, on the Sea of Okhotsk, the capital of a rich gold-bearing region, but a place of sinister reputation, because of its deadly climate and even worse because it was built and operated by forced labor. According to some information, Vavilov was put to work on breeding varieties of vegetables capable of growing in Magadan's climate, but this information is not certain. The release, through death, probably came in late 1942. No mention of N. I. Vavilov's name can be found in the list of living and recently deceased members published by the Academy of Sciences of USSR in connection with its 220-years jubilee celebrated in 1945.

Vavilov's life was connected so intimately with the development of genetics in the USSR, that his martyrdom is not separable from the crisis of the Russian branch of that science. The bold and comprehensive long range program of improvement of cultivated plants inaugurated by Vavilov in the USSR was meant to take more years than were given to it. This program, except for its early fruits, is probably lost. Although the available information is still too incomplete to permit a clear view of the situation, Vavilov is certainly not the only geneticist who fell victim of the wrath of the self-styled "Darwinists." And yet, it is assuredly not true that all genetic research has been suppressed in USSR, as some writers in American journals hastily asserted. To be sure, Lysenko had the temerity to demand that the teaching of genetics be discontinued in all institutions of higher learning in the USSR, but fortunately his influence was never sufficiently great outside the sphere of agriculture. As the uninterrupted flow of publications demonstrates, enough first rate genetic research is now being done to enable the USSR at least to retain one of the places of prominence which it has secured, in part owing to Vavilov's organizing and creative activity, among the nations of the world.

THE SKIN COLOR OF CHILDREN FROM WHITE BY NEAR-WHITE MARRIAGES

CURT STERN

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IT is well known that the inheritance of color differences in negro-white crosses is based on multiple genes, as first postulated by Gertrude C. and Charles B. Davenport[2] in 1910. Most textbooks present the specific hypothesis first proposed by Davenport[1] that two pairs of genes are involved which act cumulatively and with intermediate effects in heterozygotes, so that negro pigmentation may be symbolized by *AABB*, white by *aabb* and various shades of diverse hybrid pigmentation by *AABb*, and *AaBB* (dark mulatto) *AAbb*, *aaBB*, and *AaBb* (mulatto), and *aaBb* and *Aabb* (light mulatto). In a general way this hypothesis fits the data on negro-white hybrids collected by Davenport. Undoubtedly, however, it is at best only a first approximation. Pigmentation is greatly variable in either whites or negroes. While it is known that much of this variability is inherited, little information is available as to the specific genetic conditions underlying the degrees and types of pigmentation found in either group. Correspondingly limited is our knowledge of the interaction of the "minor" genes for pigment variability with each other and with the "major" ones in negro-white crosses.

This limitation is important in a question of social significance to a considerable number of individuals: may the children of persons with negro ancestry married to whites who are presumably free of such ancestry be darker than the darker parent? More specifically, will a dark child result from the union of two apparently white parents, one of whom has had negro ancestry? The answer to these questions, if deduced from the classical two-factor hypothesis, is clearly "no," and has been frequently thus stated. This "scientific" answer is in contradiction to a belief very widely held by laymen that "black" children may result from such marriages. There is sufficient *a priori* reason to distrust such belief, yet it seems desirable to establish the truth inductively. Two lines of approach are open: (1) new, careful, and extensive studies of the genetics of negro-white crosses; and (2), an investigation of alleged cases of dark children from near-white unions. Both approaches offer advantages. The first would lead to a better founded knowledge of the genetics involved, the second would specifically bear on the problem under discussion. Both should be pursued in a systematic way.

An Alleged Instance

As a small contribution to the second approach a case will be described here which has come to my attention. While lecturing on "Human Inheritance" and discussing the genetics of negro-white differences, members of the audiences told me of a case of a "black" child of two apparently white parents, one of whom had negro ancestry. In the course of two years this information was offered independently by three persons. It turned out that all three referred to the same event. The birth of the dark child was stated to have happened more than two decades ago; that is, at or before the birth of my informants, all of whom had vaguely heard about the case from their parents. After requests for names and more facts, all three reports named the same "socially prominent" family. In other respects the stories varied. According to one, the ancestry of the mother of the dark child was given as white, that of the mother's husband as Southern with white complexion but presumptive negro background. Another account assigned the color genes to the apparently white mother. In this history the "black baby" was replaced by "very dark twins." Further attempts at clarification

through additional sources of information resulted in a third version somewhat similar to but not identical to the first one given.

According to two of the recollections the appearance of the dark offspring was the basis for divorce proceedings. The child was supposedly sent away to a children's home. Details, unimportant from the genetic viewpoint, but indicative of gossipy interest in the community were added. In no case was the legitimacy of the child doubted.

Factual Basis

It has been possible to question a member of the family concerned. This man is very closely related to but not in the direct line of ascent or descent of the child in question. He has been very cooperative and quite willing to answer questions. There seems no reason to doubt his reliability and sincerity. The facts as reported by this informant are simple. Late in the last century a white man had married a woman with known negro ancestry. It is probable that one of her parents was white. Neither the details of this woman's pedigree nor her phenotype could now be established. The marriage resulted in four children who have the appearance of whites though dark, kinky hair is found in one or more of them. One of the sons, now married, has a blonde daughter.

The case of importance for our discussion is that of another offspring of the above cited marriage. It concerns a daughter who was married to a white man. They had a single child. This marriage ended in a divorce with the child going to live with his father. The informant seemed to prefer not to discuss particulars of the divorce case. It was this divorce which had given rise to widespread interest in the community. The divorce was attributed to a black phenotype of the child and the fact that the child did not remain with the mother was considered as showing that the child was "hidden away" in an institution. According to the informant, both parts of the story are completely wrong. He stated unequivocally that the child was not dark, that the divorce had no connection with the child, and that the child went to live with the father. In reference to an earlier mentioned version, the informant also was positive that no case of twins was involved.

The notes on which this report is based were made immediately after the interview with the informant. They have not been submitted to him for verification but it is believed that they are essentially correct. Here, then, is an alleged case widely known in a middle sized city, not only among those who lived as adults when the supposed events occurred, but also among their children who at that time were either infants or yet unborn, and are now around twenty years of age. Given our *mores* and the passage of a generation, the origin of the fable and its lack of substantiation is easy to understand.

It may well be worth the effort to collect, with the collaboration of physicians, clergymen and others who may encounter such stories, a sizable number of similar alleged cases and to investigate them thoroughly. Should none of them prove to be true, a popular misconception might gradually be eradicated. Perhaps some help might even be derived from such a result in combating the basic prejudices which underlie the all too popular belief.

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INTRAINDIVIDUAL VARIABILITY OF CHROMOSOME IV OF CHIRONOMUS

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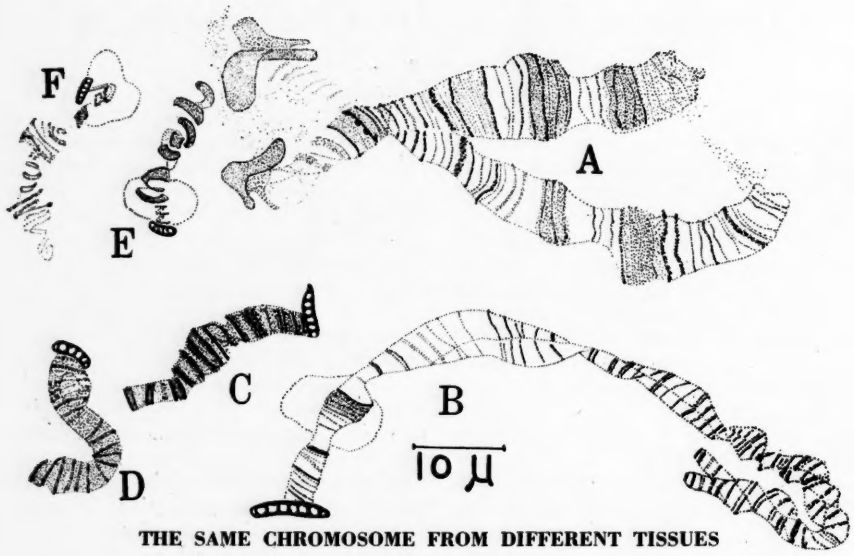
AS prototypes of giant chromosomes, those in the salivary glands of Diptera are the ones which have been the most investigated. The reason for the concentration on the salivary chromosomes is that, even if chromosomes of the giant type exist also in other tissues of Diptera, such chromosomes are smaller and therefore not so easy to examine. A recent examination of giant chromosomes in the mid-gut of *Sciara* larvae by Berger[3] led him to the conclusion that the pattern of the mid-gut chromosome is identical with that of the salivary glands of the same species although the former are smaller and some details are not easily visible for that reason.

The facts presented by Berger support the generally accepted idea that giant chromosomes correspond to enlarged interphase chromosomes whose structure and pattern is the direct evidence for qualitative differences along the major axis of the stretched chromosome.

Our examination of the pattern of giant chromosomes in different tissues of *Chironomus* are not in accord with Berger's findings. In aceto-carmine smear-preparations of larvae of *Chironomus* (called "species 2" in earlier publications) of more than 20 mm. length it is possible to find easily distinguishable giant chromosomes in mature salivary glands and also in some of the cells of the mid-gut. Chromosome IV is the smallest, and is always recognizable by the presence of the nucleolus. The pattern of salivary chromosome IV in a larva of 22 mm. is shown in Figure 2*A*. If compared with that of chromosome IV of the mid-gut of the same individual (Figure 2*B*) the different arrangement of the discs is easily visible along with some similarities such as the presence of a terminal heterochromatic disc in the head and the location of the nucleolus region. On the other hand, the diameter of the salivary chromosome is nearly twice that of the mid-gut chromosome which, however, is much longer than chromosome IV in the salivary gland. Chromosome IV in the Malpighian tubes (Figure 2*C-D*) taken from the same individual has a completely different appearance. It seems to be composed of broad bands or blocks which, on closer inspection, can be recognized as united to one another, being spirals of a coiled chromosome (or chromosomes). It is only in older larvae that Malpighian chromosome IV also shows the formation of bands in most regions, together with a remarkable elongation (Figure 3*D, E, F*). In the cells of the rectum of the same individual as that from which Figures 2*A, B, C* and *D* were also taken, the structure of chromosome IV is that of a coil with from five to seven spirals in the tail region (Figure 2*E-F*). Later, without any growth in length or breadth the rectal chromosomes are transformed into bodies with disc-like striations of a special pattern which cannot be compared directly to that of the salivary or mid-gut chromosomes. The formation of discs in chromosome IV in rectal tissue seems to indicate the beginning of disintegration of that element.

In Figure 3*A-G*, examples of this chromosome from an older larva of 25 mm. are shown. All chromosomes were taken from the same individual. The salivary gland chromosome has kept its general form and, the homologues being well synapsed, has a nearly identical pattern of bands in the two homologues. The same phenomenon is clearly visible in the much elongated chromosomes of the Malpighian tubes which, as Figures 2*D* and *E* show, are not necessarily identical in two nuclei of the same individual. But the most important fact is that, as in salivary gland chromosomes, those of other tissues also show a characteristic



THE SAME CHROMOSOME FROM DIFFERENT TISSUES

Figure 2

Camera-lucida drawings of the fourth chromosome of a 2-millimeter larva of *Chironomus*, all at the same magnification (oil immersion 100×, Ocular 9). *A*—shows paired chromosomes from the salivary gland, with the two homologues partly separated from one another, and with some differences in structure visible. The other chromosomes shown are from the following tissues: *B*—mid-gut; *C* and *D*—from the Malpighian tubules; *E* and *F*—from the rectum. The structural differences are characteristic of the chromosomes in these tissues.

pattern of bands or rings, as well as some regions which seem to remain coiled.

It is easy to see that the pattern of the same chromosome from different tissues is not identical. In the tissue of the rectum, the IV chromosome is feebly developed, as shown in Figure 2. However it must be mentioned that although some rings or discs are developed, the mid-gut chromosome IV is very small, if compared with the elongated chromosome IV of a younger larva (as shown in Figure 2*B*). Further investigations will be necessary to decide to what extent the different age or the different environmental conditions under which the larvae may have lived are responsible for this non-identical result. At the same time it has to be kept in mind that the *function* of the two figured mid-gut cells might not have been the same. These facts seem to indicate that the pattern of bands in a giant chromosome is dependent: 1) on the functional properties of the cell, 2) on the degree of synapsis of the two partners, 3) on the individual history of the chromosome in question. It has to be remembered that, whereas the giant salivary gland chromosomes develop at nearly the same time, the transformation of the chromosomes of a resting nucleus into the giant form begins at different times in different cells of the Malpighian tubes, and that this probably occurs also in the mid-gut and rectum.

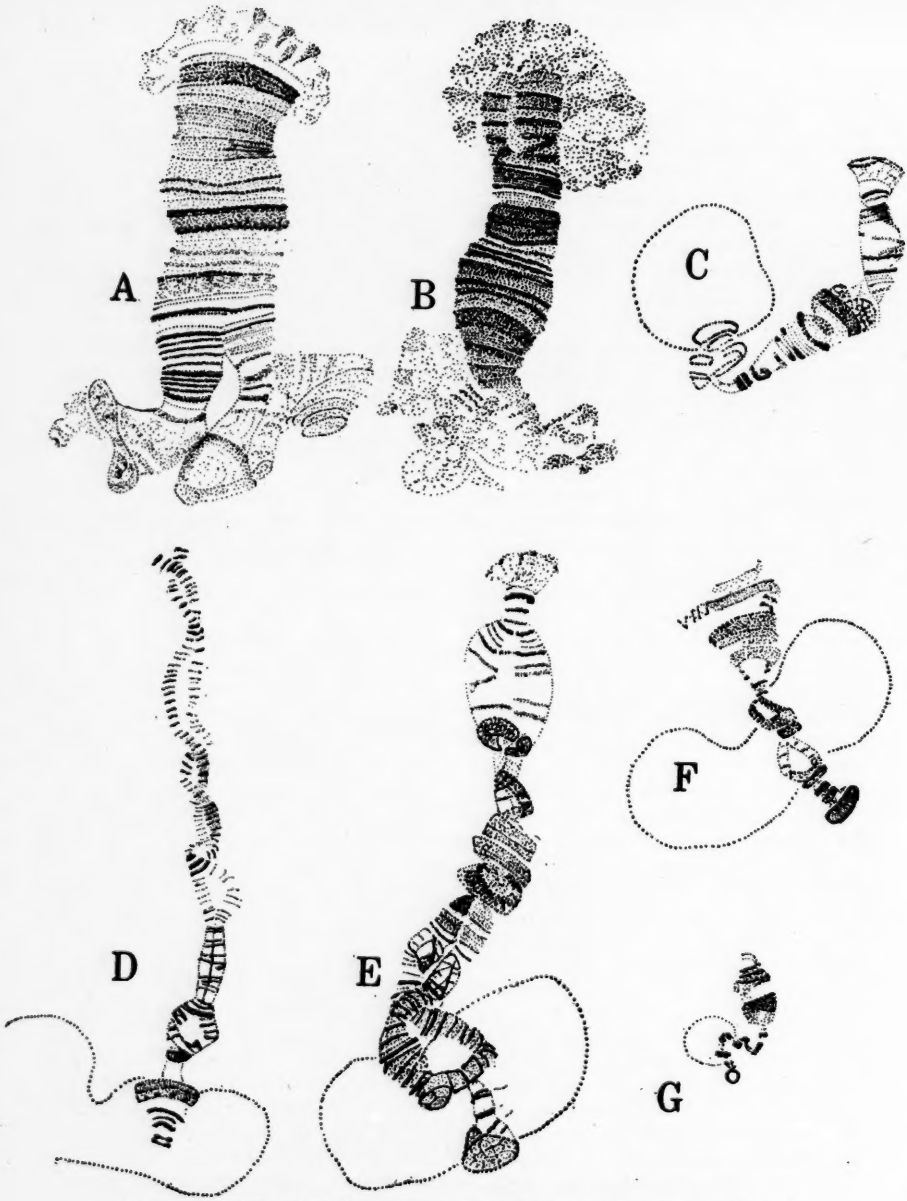
Two facts have to be taken into consideration when our results are compared with those of other authors: the intraindividual variability of the same chromosome, 1) in different tissues, and 2) in the same tissue. For the first item only internal causes are probably responsible and will be discussed below. The second item, however, seems to be in contrast to the well established con-

stancy of bands in the salivary gland chromosomes of *Drosophila.* But it must not be forgotten that, whereas the short-lived *Drosophila* larvae are kept under standard laboratory conditions, *Chironomus* larvae have to be collected from nature or from aquaria under conditions resembling those in nature with changes of room-temperature in the course of the day and of the season.

Even if, for the moment, we do not take into consideration the presence of spiralized chromosomes in Malpighian tube cells, in the rectum and, as we shall see later, in younger stages of development of salivary gland and mid-gut chromosomes, the different pattern of giant chromosomes in different tissues makes imperative a revision of our views about the structure of these elements. Up to now, the arrangement of the discs was taken as being stable and constant, only to be changed by mutation. According to the new view which we here propose, the discs seem to represent merely regions of a special type of activity (or inactivity) along the chromosome, the pattern of which is conditioned by the function of the cell in which it is housed. Identity or resemblance of structure of the same chromosome in salivary gland and mid-gut cells in *Sciara,* as reported by Berger would be understandable on the assumption that these cells had similar functions. The relation between the function of a cell and the special pattern of its giant chromosomes can be interpreted in two ways, both of which are hypothetical for the moment, but which offer possibilities for experimental proof in the future. This relation can be imagined as conditioned by the fact that the pattern developed in the chromosome is the primary and material basis by which the function of the cell is determined. In this case the chromatic discs could be thought of as an expression of inactivated genetic regions corresponding to the function of the cell. The well-known fact that genes are inactivated if transferred by translocation to heterochromatic regions, is favorable to such an interpretation. From this point of view histological and functional differentiation would be the result—at least in part—of what may be termed a "somatic mutation," reversible in all cases where differentiated cells conserve their totipotentiality. On the other hand, it could be imagined that the visible pattern of the chromosome is induced either by the differentiated cytoplasm of the same cell, or by the cytoplasmic conditions in an earlier stage of cell-lineage. Whatever the more justifiable standpoint may be, the facts presented above indicate that the discs of giant chromosomes cannot be expected under all conditions to furnish exact knowledge about the locus of a gene.

Now let us consider another fact of importance:—the coiling of the young giant chromosome. In two short publications[4,5] we noted that there are good reasons to accept the view of Alverdes[1] concerning structure and development of his so-called "Kernfäden" in the salivary gland cells of *Chironomus.* With some variations, the same course of development of giant chromosomes can be observed in different tissues of *Chironomus* and in other species of Diptera. In most cases observed by us, there is at first a coiling of the bivalent chromosome-bands: the spirals formed in this way can also give rise to secondary spirals. At a later stage, the spirals are transformed into blocks separated from one another by non-chromatic material; while these processes are going on, growth of the chromosome in length and breadth continues in a more or less pronounced fashion. In each of the blocks, the chromatic material undergoes a rearrangement which results in the formation of the discs or rings so well known in the stage of greatest differentiation of the giant chromosome. Later, these structures degenerate and disintegrate in different ways depending on the type of tissue in which they developed.

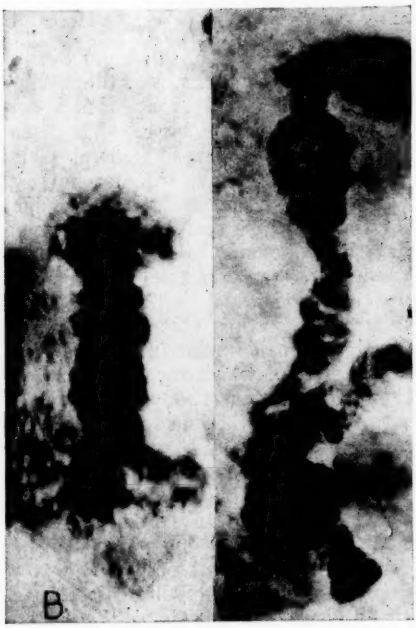
Nearly all cyto-genetic work in *Drosophila* is based on the assumption that the giant chromosomes in the salivary gland represent a bundle of stretched chromonemata. The discs are regarded



GIANT CHROMOSOMES SHOWING STRUCTURAL DIFFERENCES

Figure 3

Drawings of chromosomes from a 25-millimeter *Chironomus* larva, with the same technique and magnification as Figure 2. *A* and *B* are chromosomes from the salivary gland; *C* is the mid-gut; *D* and *E* are different types of chromosomes from the Malpighian tubules; *F* is from the distal portion of the rectum and *C* from the proximal portion. Photographs of chromosomes shown in *B* and *E* are shown in Figure 4.



SALIVARY AND MALPIGHIAN CHROMOSOMES

Figure 4

Photomicrographs of chromosomes shown in Figure 3*B* and 3*E*. The apparent branch to the right (not shown in Figure 3*E*) is another chromosome underlying the fourth.

as fixed places corresponding to the chromomeres of the single chromonema, bound together by synaptic forces. The locus of a gene is represented—a fact understandable only with difficulty from the classical point of view—by a relatively long region of the salivary chromosome containing a certain number of bands. Even overlapping of the loci of two neighboring genes could be observed. Although Metz[6,7] gives an interpretation of the structure of salivary gland chromosomes different in some wars from that of the majority of authors, in his opinion, too, the giant chromosome is in a stretched and primarily uncoiled condition. It thus appears that all authors accept the visible pattern of the giant chromosome as direct evidence for the qualitative differences along the entire length of the chromosome, corresponding to its genetical map. But if Alverdes' and our views are correct, discs are formed secondarily after the coiling of the chromosome (indeed, in the case of giant chromosomes even after the break-up of the spirals), and the rearrangement of the chromatic material takes place in correlation to the function of the cell. According to this view the locus of a certain gene cannot be expected to be represented directly by one or more of the discs; on the contrary, the number, thickness, arrangement, etc., of the discs in a certain region would be merely the indications of the action of some "genic" material which had entered into the formation of certain spirals. This material would condition the pattern in which the chromatin has to be arranged perhaps after more or less complex transformative processes. In other words, the discs do not indicate the locus or the region of a gene, if such exists, in the sense of a discrete material body or a complex molecule, but are simply a by-product in a certain region of the codewriting in which we can imagine the genic activity of the chromosome to be situated.

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DESTINY BY CATCH-WORDS

THE William Alanson White Memorial Lectures are delivered each year by a distinguished psychiatrist. In 1946 Major General C. B. Chisholm, discussed "The Re-establishment of Peacetime Society."* His thesis is that surface-scratching of the problem cannot possibly solve it, and that we must start by a searching critique of our social order to bring to light those deep-hidden compulsions that tempt — nay irrisistibly drive — mankind to indulge in that ancient behavior pattern, war. In simpler times war was amply harmful, but now this gruesome pastime has reached a point of such technological perfection that a continuation of the traditional pattern of a war each generation will mean the extinction of the human race. General Chisholm sees only two alternatives: Either we spend our remaining days with a nervous collective finger on the triggers of massed batteries of atomic bomb launchers, or we do something effective about stopping wars. He refuses to believe that we humans have imagination or foresight enough to be scared out of wars by the threat of extinction. The lid-sitting school of thought which would prevent wars by taking away the weapons from aggressor nations is toying with a dangerous makeshift. Treaties, be they ever so solemnly signed and ratified are very brittle reeds. Only by an effective reorganization of human habits and of human thinking can this murderous nonsense be stopped. Three things are necessary: (1) a breathing spell from wars and (2) enough economic equalization to give us (3) the time we must have to do the only thing that will permanently save our collective skins: adultize mankind.

The three objectives are considered in ascending order of difficulty. "Elimination of the occasion for valid fear of aggression . . . is attainable at least temporarily and as a stopgap until something better can be arranged." Then, over-hopefully perhaps, General Chisholm assumes that it will not be very difficult to give mankind abundance during this breathing spell. All people must have the oportunity to live in reasonable comfort "on economic levels which do not vary too widely either geographically or by groups within a population. This is a simple matter of redistribution of material of which there is plenty in the world for everybody, or of which plenty can easily be made." Had General Chisholm really come to grips with the problem of distribution of population and of economic goods in the world, he would have recognized that this claim that an abundance might be available for everybody is part of the pattern of infantalism he is in revolt against. Actually there are not enough goods to clothe nor of food to nourish adequately the present world population of over two billion people. The available arable land of the world amounts to less than two acres per person: with an increase in population of *50 thousand people every day* there is obviously a very slim chance that a quick balance between mouths and food can be achieved. We fool ourselves if we think that the "fiat panis" of United Nations' F. A. O. can become a reality merely by the waving of a verbal wand.

In spite of these misgivings, General Chisholm's third "must" is so important that it should not be dismissed because he may have under-stressed the difficulties of achieving security and abundance. Above and beyond these other essentials, one thing we must have: "There should be enough people in the world, in all countries, who are

CHISHOLM, G. B. *The Re-establishment of Peacetime Society.* Reprinted from *Psychiatry* for February 1946. Pp. 44. $0.40. William Alanson White Memorial Foundation, 1711 Rhode Island Avenue, Washington, D. C. General Chisholm was Director General of the Medical Services of the Canadian Army, and later Deputy Minister of Health for Canada. He is President of the Canadian Committee for Mental Hygiene. At present he is Executive Secretary of the World Health Organization of the United Nations.

not as we are and always have been, and will not show the neurotic necessities which we and every generation of our ancestors have shown. We have never had enough people anywhere who are sufficiently free of these neurotic symptoms which make wars inevitable." Relax, and grow up: or after us the deluge of the disintegrating nuclei, or the quietly drifting radioactive dust.

The Goal: Maturity

What is this freedom from neuroticism which could bring salvation? Basically it is maturity. "Strecker and Appel have recently defined maturity in terms of abilities which, if attained by enough people, could ensure the continuity and continued development of the race along the lines of its inherent destiny without wars. To quote, 'Maturity is a quality of personality that is made up of a number of elements. It is stick-to-it-iveness, the ability to stick to a job, to work on it, and to struggle through until it is finished, or until one has given all one has in the endeavor. It is the quality or capacity of giving more than is asked or required in a given situation.' He can show tolerance, he can be patient, and *above all he has the qualities of adaptability and compromise.* Basically, maturity represents a wholesome amalgamation of two things: 1—dissatisfaction with the *status quo,* which calls forth aggressive, constructive effort, and 2—social concern and devotion. It is morale in the individual."

The thing that robs mankind of his maturity, General Chisholm believes, is misplaced authority and the ritualistic maintenance of traditional folk-mores that deny human beings the use of their intelligence. With the best intentions we bind ourselves and blind ourselves as we struggle to escape the use of half-measures inevitably doomed to failure.

"The crippling of intelligence by these bandages of belief, in the name of virtue and security for the soul, is as recognizable as that of the Chinese girl who was sacrificed to the local concept of beauty. . . . For a cause we must seek some consistent thread running through the weave of all civilizations we have known and preventing the development of all or almost all the people to a state of effective maturity. What basic psychological distortion can be found in every civilization of which we know anything? It must be a force which discourages the ability to see and acknowledge patent facts, which prevents the rational use of intelligence . . . which produces guilt and fear, and which makes the controlling of other people's behavior emotionally necessary. Is there any force so potent and so pervasive that it can do all these things in all civilizations? There is—just one. The only lowest common denominator of all civilizations and the only psychological force capable of producing these perversions is morality, the concept of right and wrong, the poison long ago described and warned against as "the fruit of the tree of the knowledge of good and evil."

The Retribution of Passion and Truth

Despite the clear biblical warning, says General Chisholm, we have ignored the delicate equilibrium between the moral antitheses, and have, to the enslavement of our fullest capacities, plumped for "good." In the words of E. M. Forster our collective "sin against passion and truth" brings as punishment a gruesome march of the living dead to "their destiny by catch-words." Now with geiger counters ominously clicking outside destiny's arsenal, spurned passion threatens a grim retribution indeed. In the General's vivid phrases:

> Misguided by authoritarian dogma, bound by exclusive faith, stunted by inculcated loyalty, torn by frantic heresy, bedevilled by insistant schism, drugged by ecstatic experience, confused by conflicting certainty, bewildered by invented mystery, and loaded down by the weight of guilt and fear engendered by its own original promises, the unfortunate human race, deprived by these incubi of its only defences and its only reasons for striving, its reasoning power and its natural capacity to enjoy the satisfaction of its natural urges, struggles along under its ghastly self imposed burden. The results, the inevitable results, are frustration, inferiority, neurosis and inability to enjoy

living, to reason clearly or to make a world fit to live in.

* * *

The suggestion that we should stop teaching children moralities and rights and wrongs and instead protect their original intellectual integrity has of course to be met by an outcry of heretic or iconoclast, such as was raised against Galileo for finding another planet, and against those who claimed the world was round, and against the truths of evolution, and against Christ's reinterpretation of the Hebrew God, and against any attempt to change the mistaken old ways or ideas. The pretense is made, as it has been made in relation to the finding of any extension of truth, that to do away with right and wrong would produce uncivilized people, immorality, lawlessness and social chaos. The fact is that most psychiatrists and psychologists and many other respectable people have escaped from these moral chains and are able to observe and think freely. Most of the patients they have treated successfully have done the same and yet they show no signs of social or personal degeneration, no lack of social responsibility, no tendency toward social anarchy. This bugbear has no basis in fact whatever. We all recognize these reactions as those of the immature, the inferior, the guilty, which are not found in the mature, integrated personality. Freedom from moralities means freedom to observe, to think and behave sensibly, to the advantage of the person and of the group, free from outmoded types of loyalties and from the magic fears of our ancestors.

* * *

The most important thing in the world today is the bringing up of children. It is not a job for economic or emotional misfits, for frightened, inferiority-ridden men and women seeking a safe, responsible and quickly attainable, social and emotional status, nor for girls filling in their time before marriage. Fortunately there are recent signs of intellectual stirrings amongst teachers which give some hope. To be allowed to teach children should be the sign of the final approval of society. The present scale of values is clearly illustrated by the disparity between teachers' salaries and those of movie actresses or football coaches. I am reminded of a group whose responsibility was the reclamation, training and rehabilitation of all the unmarried mothers in a certain community. The procedure was to have an "I.Q." done and then to train the girl according to a simple chart. The upper levels rated various types of useful training. Those at the bottom, not fit for anything else, were trained as nursemaids, to bring up children. Thus, hundreds of defenceless children in that large community have been brought up by moronic unmarried mothers. Because these are psychopathological matters, psychiatrists simply have to take the responsibility of interpretation and initiative.

* * *

Let us be our own authority. We know far more than any of our ancestors. Scientists of this generation have no obligation to admit superiority of knowlege or of wisdom in any body of traditional belief or authority. There is no room for authoritarian dogma in the field of human relations. Let us discard the bromides which have kept us drugged, obedient to the old people and afraid of their displeasure. Let us accept our own responsibility to remodel the world in bolder, clearer, more honest lines. Let us stop prostituting man's noblest and highest development, his intellect, to the service of guilt and fear and shame.

We have learned to raise pigs and cows and horses, even to grow flowers and vegetables, in ways that make them of greater service to mankind. If your son is going to raise hogs for a living he goes to a college for three or four years to study under experienced teachers. But if he is merely raising children he commonly learns nothing; nor appreciates, even dimly, that there is anything that he has tc learn. Surely the rearing of children is greatly more important, and more complicated, than the raising of pigs. Within the possible expressions of human nature are the personalities of a Caligula or a Franklin Roosevelt, a female guard at Belsen Camp or a Florence Nightingale, a Hitler, or, almost, a Christ. Is it not important to us which we produce? Is not this the great problem facing our generation? Is there any greater field for earnest, devoted work? Whatever will help our children to grow up to be mature, compassionate, tolerant and worthy of being citizens in the world they must make, must be of interest to psychiatrists—but not to psychiatrists, alone—and part of their training in the future. Psychiatrists must, with all the help they can get from every source, become specialists in living. We have our chance now. If now we all revert to our little private concerns, if we all tell ourselves it is someone else's responsibility, there will one day be none of us left, not even any to bury the dead.

Needed: A Human Ecology

In the evolutionary hierarchy, the unique quality of human beings is their adaptability, their inborn ability to be moulded by the social environment into near-Hitlers or into near-Nightingales. We condition each generation to their roles by rearing the young in fantastic worlds of make-believe. Fairies, the bogy-man, Santa Claus, the stork have their protypes in every culture, and each of us runs a fantastic gantlet of semantic confusion as we journey toward chronological maturity. "Loaded" words packed with vague and ill-defined con-

notations explode like booby traps about us, threatening dire consequences if we behave adaptively instead of according to whatever book of ancient wisdom we take as our guide. So disconcerting is this weird behavior that we need not wonder that many tie off their minds and hasten into the safe arms of a precocious second infancy even before puberty has set in. This semantic concept of the strangling of the adaptive function of the new brain is more general and more fundamental than Chisholm's concept of sin. It is broad enough to account for the fact that many people, including some psychiatrists, even, who escape from ecclesiastical enmeshment still fail to achieve effective maturity. "The gods of the copybook maxims are the last of the gods to die."

Looked at in this way the problem transcends psychiatry and becomes rather a problem in human ecology—using that term in an emergent sense to include in the total behavior nexus the physical-ecological factors as well as the intellectual and emotional factors in the social *milieu*.

This is not an easy or simple undertaking, and in embarking on it there is danger that we will get into trouble if we over-stress the exalted status given us in the knowledge we possess as compared with that of our predecessors. A little knowledge—which is all we have in a total context—can be very dangerous and what we have hardly justifies much smugness. Knowledge, alas, is not wisdom and is not always a substitute. Part of our present predicament, Leonard Bacon points out, is that "wisdom has dwindled to fact." Facts are indispensable, but of themselves they won't do the job. Fact-"wisdom" led the Germans to the Belsen-guard psychology: It could happen here too.

As an example of the morasses into which a substitution of fact for wisdom can lead us, consider the predicament our army engineers have achieved through their attempt to control floods by building levees, when the sickness of our rivers lay not in the flood-plains but in eroding watersheds. Today many of our rivers are rising above the land on stilts of silt. So now the fact-wise are demanding the flooding of square miles of arable land up-stream to protect down-stream acres which in the end will be inundated by floods sure to top any man-made barriers. Such relatively simple ecological complexes as the control of floods need wisdom as well as knowledge for their resolution, and fact-wisdom can be depended on to make monkeys of us in the end.

When we attempt an over-all attack on the front opened up by General Chisholm, the development of adequate and humane tools to do the jobs of reintegration is a vastly complex matter. We must develop an effective and realistic philosophy of human ecology that will take account not only of the need for banishing bogus fears and obsessions, but also will face courageously the huge job of banishing the very real biological threats—the certainty of privation and starvation of the body as well as of the mind and of the spirit of man. Such overt and hidden hungers a majority of human beings face today as a consequence of our pathological adolescence—of our steadfast refusal to square the geometry of reproduction with the arithmetic of land areas. We know that in an evolutionary context "virtues" and "vices" do bring their own reward—perpetuation or extinction of the species. In a context of mature wisdom, these terms might achieve far greater realty and significance than we dare hope today. But semantic subterfuges and ritualistic make-believe cannot save us.

Between Two Worlds

It is no detraction from General Chisholm's presentation to point out that our trouble in no small measure stems from the fact that today we live in a period of transition between two dispensations—two conflicting and mutually exclusive universe concepts. Behind us lie the ruins of the cozy anthropomorphic universe of Bishop Usher, which sprang fully clothed out of chaos, darkness, and

void one fine week in 4004 B.C. In that forgotten halycon time of fixed reference-points, eternal absolutes and no nonsense, life was real, earnest, and *simple* in a way that we dwellers in the windless vastness of an expanding universe of shifting relativities can scarce imagine. In our departure from the Eden of the Victorians we have for the second time been thrown out of the Garden. An angel with a flaming sword of reality makes it impossible for us to clamber back onto the Gospel Train, running on schedule "toward that far-off divine event to which the whole creation moves." Evolution we know today is precisely the reverse of inevitable progress: It is a balancing on knife-edges. Even that smug hope that "nature" is concerned about the species though careless of individuals we know is nonsense. In that happy time when evolution was inevitable we could afford to be childish, and the price was not unbearably high. Now, thanks to the inflation of nuclear physics, the price has been raised to include the lives of all of us. Even a cliché is pardonable when it vividly states a truth: "It is much later than we dare to think!"

Is not the existence of this deep transitional dichotomy the basis of much of the infantile behavior which alarms General Chisholm? To what extent is this persisting intellectual schizophrenia responsible for our lack of integration and fruitful imagination, for the confusion and insecurity which is manifested as a well-nigh universal need to dominate the lives of others? The fact that we are still enmeshed in so all-embracing a conceptual change-over robs us of perspective we must have for a clear view of what is happening to us. A basic essential ingredient for the flowering of an integrated culture is that the universe-concepts which are presented to our minds by the sum of knowledge are reasonably and satisfactorily congruent with the mores and social imperatives by which we chart our collective lives.

This job is not one that the psychiatrist can undertake alone. Part of it is in the realm of art; part of it is in the realm of biology and of genetics, of comparative anthropology and psychology. Only by an integration of all significant social forces and life-forces will we achieve release from infantile obsessions. In working toward this integration, a sane and balanced eugenic philosophy must make an essential contribution. In such a context eugenics would gain a perspective it now lacks. Its tendency to be over-obsessed with pedigree and class is analogous to the over-emphasis placed on individual salvation in a man-centered universe. Galton conceived eugenics to be a study of the forces under social control which might be manipulated to enhance the inborn genetic qualities of the human race. In the total context this means that we must consider the effects of selective forces on the "gene-pool" of the total population. Looked at this way, eugenics is an essential ingredient in an overall plan to achieve the mature integrated world of General Chisholm's dream. In such a world the continuance of trends inevitably doomed to result in the physical or mental deterioration of human beings would hardly make sense. In a world of grown-up infants even the much more urgent danger of mass starvation is not a matter of any compelling significance.

General Chisholm has put the real cards on the table, and face up. Our salvation is not to be found at Lake Success, in the bureaucratic rabbit-warrens of the Federal Triangle in Washington, at Whitehall or in the Kremlin. A being capable of reason has the question squarely put. Human gadgeteering at an adolescent level means the dead unburied. Human ecology and engineering, and a mature and enlightened teaming up of the skills now available *could* mean "no limits." The question is: Are those chains of the mind so tightly anchored that we cannot help ourselves, even though we see the fuse sputtering and know that time is short?

—R. C.

FLESH COLOR IN HYBRIDS OF TOMATO

Sub-Generic Crosses Indicate That Three or More Genes Determine Red-Yellow Color Series*

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THE data reported in the present paper were obtained in the course of an attempt to produce new disease-resistant tomatoes by species hybridization. The domestic tomato (*Lycopersicon esculentum*) or hybrids between the tomato and the cherry tomato (*L. pimpinellifolium*) were crossed with two species† of the sub-genus *Eriopersicon,* namely *L. hirsutum* and *L. peruvianum* var. *dentatum, L. hirsutum* is tolerant to *Verticillum* wilt and tobacco mosaic, and certain derivatives of *L. peruvianum* var. *dentatum*‡ are klendusic§ to that virus.[4] It was hoped to combine these important characteristics in an acceptable tomato variety.

Wild-growing races of *L. esculentum* and *L. pimpinellifolium* have red-fleshed fruits which contain lycopene and several yellow carotenoid and other pigments. These races carry the dominant alleles (*T R*) of tangerine (*t*), and yellow (*r*). In the cultivated varieties slight differences in color of red-fleshed fruits are reported by Boswell *et al.*[2] Without careful comparison of fruits at various stages of development, these might easily be overlooked, but red, yellow, and tangerine are usually easily distinguishable.

L. hirsutum is a native of northern Peru and of Ecuador, where it occurs at elevations of 7,000 to 8,000 feet. The flesh of the fruit is greenish white and evidently contains no lycopene; the skin contains practically no yellow color. This species blooms profusely but is very unfruitful at Riverside, California. It may set fruit in late summer or early autumn.

L. peruvianum also has small greenish-white fruits with no red color in the flesh. This species is a native of southern Peru and northern Chile. It occurs in the coastal region at elevations ranging from about 100 to 6,000 feet. It grows poorly at Riverside, blooms sparingly, and is also very unfruitful.

A Scale for Fruit Color

Besides the yellow and non-yellow types of skin color of *L. esculentum,* several intermediate grades of yellow occurred in the hybrids developed in these studies. It was therefore necessary to peel the fruits in order to determine the flesh color.

Seven different phenotypes, hereafter referred to as types 1 to 7, were identified by comparing the flesh color of the outer rind of the mature fruit with the colors in Maerz and Paul's *Dictionary of Color.*[11] The color designations most nearly corresponding to the types identified are listed below:

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†The relationships of the genus *Lycopersicon* are given by C. H. Muller[12] in a recent revision, as follows:

genus:	*Lycopersicon*	
subgenera:	*Eulycopersicon*	*Eriopersicon*
species:	*L. pimpinellifolium* (Jusl.) Mill. *L. esculentum* Mill.	*L. hirsutum* Humb. and Bonpl. *L. peruvianum* (L.) Mill. *L. cheesemanii* Riley *L. glandulosum* C. H. Mull. A fifth species is added by Luckwill[9], namely *L. Pissisi* Phil.

‡For brevity, references in this text to *L. peruvianum* var. *dentatum* are condensed to "*L. peruvianum,*" as this variety is the only one used in these experiments.

§Klendusic, coined by Holmes,[4] means "characterized by a tendency to escape infection."

Type no.	Maerz and Paul[11] designation Plate	Column	Row
1 (Least red)	10	K	9
2	3	B	11
3	3	H	11
4	3	K	11
5	3	J	9
6	3	L	9
7 (Most red)	4	L	5

These types constitute merely a nominal scale. Although only seven distinct types were readily distinguishable by mere eye estimation, the actual number may have been much larger. The intensity of both red and yellow colors varied. Type 1 contained the least red and type 2 next to the least; type 7, which includes the common red-fleshed tomato, contained the most red. Types 3 and 4 may have contained as much red as type 5 but they seemed to contain much more yellow.

Color Types from *L. esculentum* × *L. hirsutum*

Two small F_1 families were obtained from this cross, using two seedlings of *L. hirsutum* (U. S. Dept. of Agri., P. I. 126447) as the pollen parents. The reciprocal crosses with *L. hirsutum* as seed parent failed. One *L. esculentum* parent was homozygous and the other heterozygous for the yellow flesh gene *r*.

F_1 plants from both crosses produced a few fruits out-of-doors at Riverside, California. The fruits averaged about four grams in weight, and in flesh color were about type 1, or ochraceous buff, like those described by Porte, Doolittle, and Wellman.[13] Most of the seeds were very small and were probably non-viable.

Table I summarizes the various matings and the distribution of offspring by color type. All parenthetical references in the following discussion are to the table; for example, (I*a*) refers to Table I, line *a*. A type 7 plant of *L. esculentum* homozygous for *R* (red flesh) was used as the backcross parent throughout.

In the first backcross generation (I*a*) seed germination was poor, and nearly half the plants were unfruitful, but a few were very fruitful. Of the five distinct phenotypes, none was type 7. A type 1 plant did occur in another BC 1 not shown in the table. When selfed (I*c*), again at least five phenotypes occurred, but again none was type 7. If in this family types 1 and 2 and types 4 and 5 are combined, a ratio of 24:6:13 is obtained, or about 9:3:4. This suggests that this plant may be heterozygous for two independent genes.

The occurrence of all seven phenotypes in one instance (I*d*) in so small a population suggests that this BC 1 parent may be heterozygous for at least three pairs of alleles. However, the phenotype differences were sometimes so small that classification was difficult.

The disparate distribution of phenotype in the type 7 × 4 cross (I*g*, *h*) indicates a possible genetic difference in the type 7 parents. (In I*h*, the *esculentum* was of a different race from the one in I*g*).

The next type 7 × 4 mating (I*i*) produced equal numbers of types 3 and 7. When this same type 4 was selfed (I*j*) it produced eleven types 3-4 to four type 6. One of these type 4 offspring, backcrossed (I*k*), gave the parental types in about equal numbers. Here the phenotypes were readily distinguishable, and no intergrades occurred. It is interesting to note the increased incidence of type 7 in the offspring (I*m*, *n*, *o*), as the parental types go up the color scale, until in the instance of type 7 selfed, all the progeny are of this type.

TABLE I. — Flesh color of fruit in families from *Lycopersicon esculentum* × *L. hirsutum* backcrossed to type 7 *L. esculentum*.

Ref. no.	Generation of progeny	Parental color type†	Number of offspring by color type† 1	2	3	4	5	6	7
a.	BC 1	1×7	....	1	7	1	5	4	....
b.	BC 1 selfed	5	....	....	....	....	2	2	....
c.	BC 1 selfed	1	2	22	6	....	3	10	....
d.	BC 2	1×7	1	5	11	3	2	16	10
e.	BC 2 selfed	3	....	....	50	4	....	....	25
f.	(BC 2 selfed) slf'd	3	....	....	33	....	....	....	15
g.	BC 3	7×4	....	....	22	....	....	....	20
h.	BC 3	7×4	....	....	....	43	....	21	....
i.	BC 3	7×4	....	....	10	....	....	....	10
j.	BC 2 selfed	4	....	....	6	5	....	4	....
k.	*e×(BC 2 selfed)	7×4	....	....	....	7	....	....	9
l.	BC 2 selfed	6	....	....	....	6	....	6	....
m.	e×(BC 2 selfed)	7×4	....	....	....	....	....	11	8
n.	BC 3	6×7	....	....	....	....	....	9	11
o.	(BC 2 selfed) slf'd	7	....	....	....	....	....	....	43

* e = type 7 red-fleshed *L. esculentum*.
† For explanation of color types, see text above.

Three Modifiers of Red?

The data indicate that *L. esculentum* and *L. hirsutum* differ in at least three genes modifying red flesh color, and that each of these genes has only a slight visible effect. Apparently, the alleles which inhibit lycopene formation come from *L. hirsutum* and are partially dominant, since the plants obtained from selfing or backcrossing any given type to type 7 *L. esculentum* as a rule have at least as much red pigment as the parent or parents. Some genes or gene combinations decrease yellow flesh color, but there is evidence that yellow pigmentation is even more difficult to estimate visually. Thus, by the chromatographic method, Le Rosen, Went, and Zechmeister[5] found that the common red-fleshed type of *L. esculentum* contains more yellow carotenoids than the yellow-fleshed *r* mutant.

Gradations in skin color occurred between the dark yellow of *L. esculentum* and the almost colorless skin of *L. hirsutum,* and between the non-uniform and uniform green coloration of the immature fruit, determined by the *U*-alleles in *L. esculentum.* In one family originating from *L. esculentum, L. pimpinellifolium,* and *L. hirsutum,* some plants developed a red flesh color which seemed more intense than that of any of the parents or hybrids of *L. esculentum* and *L. pimpinellifolium.* Apparently a new combination of genes causing a more intense red was brought together by crossing.

Color Types from *L. esculentum* × *L. peruvianum*

The flesh color of the fruit of the F_1 hybrid of *L. esculentum,* or of F_1 *L. esculentum* × *L. pimpinellifolium* crossed with *L. peruvianum* was classified as type 1 in the nominal scale already described. The pale-greenish flesh color of *peruvianum* was partially dominant. The diploid F_1 plants set very few fruits from open pollination, but one of the tetraploid F_1 plants, although sterile for three seasons, set a few seedy fruits late in 1944 and many in 1946. Six seedlings from this plant were tetraploid and were similar to the parent in appearance. Diploid F_1 plants were backcrossed, as pollen parents, with *RR* plants of flesh color type 7 similar to those of the P_1 generation. Table II shows the types of flesh colors observed in families derived from these crosses.

Unfruitfulness was more frequent than in the *esculentum* × *hirsutum* hybrids. About 25 per cent of the BC 1 plants set a very scanty crop late in the season. At least four types of flesh color occurred in a small BC 1 family, ranging from type 2, which was slightly redder than the F_1, to type 6, which is less red than *esculentum* red. Probably two other phenotypes occurred which differed appreciably in the amount of red pigment. Several other small BC 1 families were grown in the earlier years, but the records as to color are not comparable with those obtained in 1944 and 1945. Young[15] noted the occurrence of fruit colors in the BC 1 that were not found in *L. esculentum.*

Table II summarizes the various matings and the distribution of offspring by color type. All parenthetical references in the following discussion are to the table; for example, (II*a*) refers to Table II, line *a*. In all backcrosses a red parent (type 7) was used throughout. In BC 1, color distribution of the offspring ranged along the color scale but did not extend to the extreme types 1 and 7 of the parents (II*a*). BC 1 selfed gave 60 plants of which only 25 were fruitful. The offspring were distributed over four color types, two of the plants being less red than the parents. There were many intergrades of the phenotype which were difficult to fit into our color scale. Thus in type 3 × type 7 matings (II*c, d*), offspring were of color type 4 and 6 and included several intergrades between these types. Color of offspring of type 7 × 4 matings (II*e, f, g*), and of type 6 × 7 matings (II*i, j, k*) was of parental types, with an occasional type 4 appearing among the progeny of the latter. All type 7 × type 7 matings (II*l, m.*

t, *u*, *v*. *w*) produced offspring of the type 7 phenotype with one exception (II*w*) where some of the offspring were type 6.

Some BC 2 and BC 3 plants were selfed for more than two generations. The results suggest that low intensity of red color is partially dominant. Type 3 selfed (II*n*) produced progeny almost half of which were of a redder color than the parent. Similarly, some type 4 parents selfed gave offspring of the parental color type or redder (II*q*), although others gave progeny of types 2 and 3 as well (II*o*, *p*), indicating incomplete dominance of the less red flesh type. As a rule type 6 plants selfed (II *r*, *s*) gave type 6 or type 7, with no progeny less red than the parents.

A novel distribution of flesh-color also occurred in this species cross. Normally, the red flesh color is fairly evenly distributed, or tends to be more intense in the rind than in the tissues adjacent to the seeds, but in one BC 4 plant, when growing in a greenhouse, the red color was most intense in the cell layers lining the loculi, and in the tissue surrounding the seeds. Seed obtained from selfing this plant germinated poorly but gave one plant similar in fruit color to the parent.

In a small family originating from a BC 1 plant selfed, some young plants in the field were partially chlorotic. The petioles and basal portions of the young leaves were yellowish but became more normal in color later in the season. One quarter of the plants in this family were affected. At Riverside, California, *L. peruvianum* tends to a generally chlorotic condition. This condition did not occur in any adjacent family and is probably due to a recessive gene.

TABLE II. — Flesh color of fruit in families from *Lycopersicon esculentum* × *L. peruvianum* backcrossed to type 7 *L. esculentum*.*

Ref. no.	Generation of progeny	Parental color type†	Number of offspring by color type†						
			1	2	3	4	5	6	7
a.	BC 1	1×7	....	1	1	....	2	6	....
b.	BC 1 selfed	2	2	11	10	2	....	....	....
c.	BC 2 selfed×e¹	3×7	....	....	....	16	....	....	....
d.	BC 2 selfed×e²	3×7	....	....	....	55	....	12	....
e.	BC 3	7×4	....	....	....	6	....	....	13
f.	BC 3	7×4	....	....	....	12	....	....	8
g.	BC 3	7×4	....	....	....	8	....	....	11
h.	e×BC 2 selfed	7×4	....	....	4	6	....	5	1
i.	BC 3	7×6	....	....	....	1	....	16	....
j.	BC 3	7×6	....	....	....	....	....	13	6
k.	BC 3	6×7	....	....	....	....	....	20	....
l.	BC 3	7×7	....	....	....	....	....	....	20
m.	e×BC 2 selfed	7×7	....	....	....	....	....	....	16
n.	BC 2 selfed	3	....	....	20	16	1	1	....
o.	[(BC 2 selfed)×e] selfed	4	....	....	9	20	....	16	4
p.	(BC 2 selfed) slf'd	4	....	7	....	10	....	3	....
q.	BC 3 selfed	4	....	....	....	9	....	....	7
r.	BC 3 selfed	6	....	....	....	....	....	13	3
s.	BC 3 selfed	6	....	....	....	....	....	16	....
t.	BC 3 selfed	7	....	....	....	....	....	....	16
u.	BC 3 selfed	7	....	....	....	....	....	....	16
v.	BC 3 selfed	7	....	....	....	....	....	....	16
w.	BC 3 selfed	7	....	....	....	....	....	9	17

*e, e¹, and e² = red-fleshed types of *L. esculentum* or of *L. esculentum* × *L. pimpinellifolium*.
†For explanation of color types, see text page 246.

Unfruitfulness

The unfruitfulness of the diploid F_1 and of many BC 1 families, and to a lesser extent of some of the BC 2 and BC 3 families, suggested the possibility of irregularities in meiosis. The meiotic chromosomes of *L. peruvianum* have a marked tendency to non-disjunction in hot weather[7] resulting in the production of a very high percentage of diploid gametes. This variety is very unfruitful outdoors and in the greenhouse at Riverside. All varieties of *L. esculentum* are much more stable cytologically.

Both *L. esculentum* and *L. peruvianum* have twelve pairs of chromosomes. In the F_1 two pairs are often weakly associated, but twelve meiotic pairs are usually seen. This weak association has also been observed in later generations. In midsummer the most sterile plants—and in one population from a BC 4 plant selfed all plants studied—produced large percentages of diploid gametes. Only pollen mother cells have been studied, but if, as is probable, the embryo-sac mother cells behave in the same way, fertility would certainly be greatly reduced thereby. This tendency to cytological instability indicates that *L. peruvianum* and many of the hybrids are physiologically ill-adapted to fruit-setting under Riverside condition, but does not explain why some plants are wholly sterile, since many normal-looking meiotic divisions occur in most anthers of all plants observed, and at times reduc-

tion can appear to occur quite normally.

Evidence reported by Luckwill[9] suggests that photoperiodism may influence the blooming and fruitfulness of *L. hirsutum* and *L. peruvianum* when they are grown at Riverside, California. This condition may also affect the fertility of the derivatives of these species crosses. *L. esculentum* is reported not to be very sensitive to changes in the photoperiod. Went[14] has shown that night temperature influences fruitfulness, and differs in its effect on different varieties of *L. esculentum*. In our cultures *L. esculentum* varieties growing under similar conditions varied considerably in fruitfulness, but none was as unfruitful as some of the species hybrids. Both photoperiod and night temperature may account in part for this unfruitfulness.

Discussion

In *Lycopersicon esculentum* the two independent mutant genes for flesh color, *r* and *t*, are almost completely recessive to their wild type alleles. The mutant types yellow *rr* and tangerine *tt* contain practically no lycopene and are very distinct from red, the wild type.

By the chromatographic adsorption method, Le Rosen, Went, and Zechmeister[5] and Le Rosen and Zechmeister[6] studied the effects on flesh color of the genes for red, yellow, and tangerine in *L. esculentum*. They found that the single gene difference between red and yellow flesh causes quantitative differences in several pigments, and that about 60 per cent of the carotenoid pigment in red flesh is lycopene. A preliminary chromatographic analysis by Professor L. Zechmeister, of the California Institute of Technology (unpublished), indicates that a small amount of lycopene is present in the pale-red-fleshed fruits of F_1 *L. esculentum* × *L. peruvianum* var. *dentatum*, which was type 1, and in that of the two BC-1 plants of (*L. esculentum* × *L. hirsutum*) × *L. esculentum*, classified as types 2 and 3. Most of the pigment in these three cases was carotene. In the type 2 BC 1 plant 90 per cent of the total pigment was β-carotene, and in the type 3 BC 1 plant, 75 per cent.

In the BC 1 generation (*L. esculentum* × *L. hirsutum*) × *L. esculentum*, analyses by Lincoln *et al.*[8] obtained by adsorption and spectroscopic observation, disclose a wide range in carotenoid content; some plants contained a higher concentration of β-carotene and almost as much lycopene as the *esculentum* varieties.

There is a marked similarity in the genetic relations between these two species crosses reported here. It appears that both *L. hirsutum* and *L. peruvianum* differ from *L. esculentum* in at least three genes affecting flesh color. The actual number of genes and their identity is unknown, and on account of errors in classification of flesh color only the general trend of the relationships between the various types was clear. They appear to be modifying genes with a cumulative effect tending to reduce the concentration of lycopene and, to some degree, that of the yellow pigments. Most of the progeny from selfing or backcrossing had at least as much red color as the parents. Numerous gradations in pigmentation occurred in the first backcross and later generations, between a red tinge and full red equal in intensity to that of the red-fleshed *esculentum* type used in backcrossing. Accordingly, a number of new genetically distinct phenotypes have originated from these interspecific crosses. Extracted full-red types almost always bred true. Type 6 and type 7 derived from *L. esculentum* × *L. peruvianum* may differ in a single modifying gene. In BC 1 and in many other families numerous intergrades occurred which were difficult to classify, but in some families from subsequent backcrossing and selfing, a few easily distinguishable flesh-color types were found.

The study of interspecific hybrids of *Antirrhinum* led Baur,[1] in 1924, to the conclusion that interfertile species differed in a large number of genes which had a relatively small individual effect.

The genetic complexity of flesh color in interspecific crosses of *Lycopersicon* seems to have a similar basis.

Harland,[8] in his comprehensive studies of interspecific and intraspecific hybrids of *Gossypium,* found that the crinkled dwarf character behaved as a simple recessive in *G. barbadense* but showed a blending type of inheritance in certain interspecific crosses. He also found that in *Gossypium,* by repeated backcrossing, fertility was restored; and that eventually in some lines a clear and simple type of inheritance of crinkled dwarf, like that within certain species, emerged.

Whereas red flesh color is a normal dominant character in *L. esculentum,* crinkled dwarf is a mutant type in *Gossypium.* Harland's analysis[8] of *Gossypium* suggests that *L. esculentum* is homozygous for certain genes modifying flesh color, so that a few clearly separable types segregate out, but that *L. hirsutum* and *L. peruvianum* contain a different set of modifying genes. When these species are crossed, recombination of two different sets of modifying genes gives a new series of flesh colors. Backcrossing to *L. esculentum* causes some families to become homozygous for one set of modifying genes, so that a clear segregation of color types and increased fertility result. The similarity of the flesh colors that result from crossing *L. esculentum* with *L. hirsutum* and *L. peruvianum* suggests that the last two species have a similar system of modifiers, and that they are more closely related to each other than to *L. esculentum,* which belongs to the subgenus *Eulycopersicon.*

The existence of a truly wild red-fruited species, *L. pimpinellifolium,* suggests that the immediate antecedents of *L. esculentum* were red-fleshed; but both species may possibly have originated from a non-red-fleshed prototype. It was not determined whether *L. hirsutum* and *L. peruvianum* contain the allele for red flesh, but the absence of yellow and tangerine flesh colors in the families obtained suggests that the *r* and *t* alleles are not present in those species.

Summary

Lycopersicon hirsutum differs from *L. esculentum* in at least three genes affecting flesh color of the fruit. The alleles in *L. hirsutum* are partially dominant and have a cumulative effect, so that numerous gradations of flesh color occur; but only the general trend of the genetic relations could be inferred from the visual method of classification used in these studies. Very similar genetic relations seem to exist between *L. esculentum* and *L. peruvianum* var. *dentatum.* After backcrossing to *L. esculentum* for several generations, in some families from both species crosses, instead of a series of intergrades, a few types occurred that were almost as easily distinguishable as the red, yellow, and tangerine types in *L. esculentum.* The similarity of the data to some of those observed by Harland in *Gossypium* suggests that the *Lycopersicon* species also have different systems of genes modifying flesh color. Probably *L. hirsutum* and *L. peruvianum* var. *dentatum* are more closely related to each other than to *L. esculentum.*

The F_1 and some BC 1 plants from both species crosses were partially sterile, but many BC 2 and BC 3 families were quite fertile. Meiotic irregularities were observed but probably do not wholly account for the partial sterility. It seems more likely that this sterility is due to gene combinations which are ill-suited to the conditions of light and temperature which prevailed.

Since this paper went to press, MacArthur and Chiasson[10] have concluded from a cytogenic study of *Lycopersicon* that "Evolutionary divergence and establishment of isolating mechanisms have evidently occurred in this group, principally by gene and modifier mutation. . . ."

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THE PACHYTENE CHROMOSOMES OF *SORGHUM INTRANS*

THE pachytene chromosomes of *Zea mays* constitute very favorable material for detailed studies of chromosome morphology. Another grass, *Sorghum intrans* F. Muell., has been found to have pachytene chromosomes comparable in clarity to those in maize. *Sorghum intrans* occurs in Northern Australia and appears to be restricted to the region around Port Darwin and Katherine. Although this species is not mentioned as a member of the section Parasorghum of the genus *Sorghum*, it has the somatic chromosome number of ten which is characteristic of that section (Janaki-Ammal*).

Material and Methods

Two plants were grown to maturity from seed collected near Port Darwin by W. Hartley, Council for Scientific and Industrial Research, Canberra, Australia. One plant was isolated and allowed to open-pollinate. This plant did not set any seed and had 25 per cent aborted pollen grains. By cross-pollinating, six seeds were obtained from eight florets of the second plant.

Immature panicles were fixed in a solution of absolute alcohol and glacial acetic acid (3:1) for 24 hours and then transferred to 70 per cent alcohol for storage. Temporary smears of pollen mother cells were stained with aceto-carmine. A modified alcohol vapor chamber technique was used so that slides could be kept permanently.

All five chromosomes in each of nine pollen mother cells were measured from camera lucida drawings; only four chromosomes could be measured in one pollen mother cell.

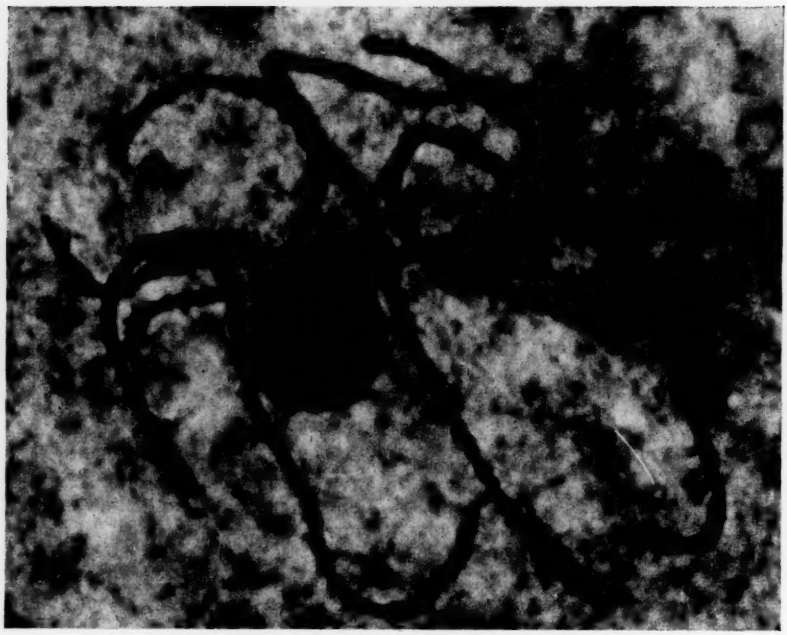
Observations

Two of the five chromosomes in a pollen mother cell were always associated with the nucleolus. Only one nucleolus, however, was found in each pollen mother cell. In somatic nuclei, from one to four nucleoli were seen and nuclei with one or two nucleoli occurred most frequently.

Unfortunately, it was not possible to locate the centromere in this material. Consequently, no information could be obtained concerning the arm ratio for each chromosome. The chromosomes not associated with the nucleolus did not seem to differ significantly in length. Although these chromosomes appeared to have certain differences in stainability, these differences were not completely reliable. Knobs found in the pachytene chromosomes of *Zea mays* were not found in *S. intrans*. Consequently, the decision concerning the value of differences in stainability as a means of identifying each chromosome must be deferred until additional material, now growing, can be studied.

The two chromosomes associated with the nucleolus are the shortest in the

*JANAKI-AMMAL, E. K. *Nature* 146:839. 1940.



SORGHUM CHROMOSOMES

Figure 5

The bivalent chromosomes of *S. intrans* at pachytene. The chromosome crossing the nucleolus is associated with the nucleolus as is the chromosome shown attached to the nucleolus at "twelve o'clock." This pollen mother cell is selected to show the clarity of the pachytene chromosome preparations. *Sorghum intrans* comes from Australia. Its chromosomes may make it valuable for cytological studies.

complement. Each may be identified by using the point of association with the nucleolus as a point of reference to divide the chromosome into two segments. In chromosome *N*, one segment is usually from two to three times as long as the other segment; in chromosome *N'*, one segment is usually only a little longer than the other segment. The following measurements represent actual lengths in micra: *N*— 48.6 ± 2.2; and *N'*— 37.6 ± 2.1.

Discussion

The association of two chromosomes with one nucleolus in prophase I of meiosis is not common in a diploid species. Whether this one plant of *S. intrans* is representative of the entire species cannot be determined until additional material is examined.

Although *S. intrans* has a diploid chromosome number of ten, the chromosomes of this species are very different in morphology from the chromosomes of other species of *Sorghum*, thus far examined, with the same diploid chromosome number (unpub.).

The low number and unusual clarity of the pachytene chromosomes of *S. intrans* make this species extremely favorable material for cytogenetic study. However, it will be necessary to determine what environmental conditions are required to insure seed set.

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RH ANTIBODIES IN MOTHERS OF FEEBLEMINDED CHILDREN*

MARY LOUISE L. SCHOLL, WARREN E. WHEELER AND LAURENCE H. SNYDER

THE question of the existence of a positive relationship between Rh immunization and feebleminded-ness has been discussed previously in this JOURNAL.[5,6] On the basis of statistical probability in a small sample the existence of such a relationship was strongly suggested. The final acceptance of this theory would require immunological confirmation and a systematical experiment to establish or disprove the reality of this relationship which will require considerable time and a larger population than has yet been studied. If such an experiment is to be conclusive it should include the following factors:

1. The case material should include a much larger number of patients, preferably more than 25 Rh positive children from Rh negative mothers.
2. The immunologic studies should be performed as soon as the child is found to be mentally deficient, preferably within one year following delivery.
3. Subsequent children in the families should be studied for evidences of erythroblastosis at birth. This would necessitate postponing the publication of the study for a number of years until such data were accumulated.
4. The criteria for the classification of the feeblemindedness should be stated (such as proposed by Yannet[9]).
5. Data on the occurrence of other neurologic disorders such as athetosis, convulsions, and spasticity, which are known to accompany kernicterus, should be stated.

This report is limited to the patients comprising Snyder, Schonfeld and Offerman's reports[5,6] wherein a statistical relationship between Rh negative mother and feebleminded offspring was demonstrated. The entire series was re-studied.

The classification of the mental deficiency was carefully checked; the mothers were visited and a thorough family and obstetrical history obtained. A sample of maternal blood was withdrawn in each instance for antibody studies. Thirteen of the sixteen mothers in the original series were located. Only two of these mothers gave an obstetrical history suggestive of Rh immunization and in only one of these could anti-Rh antibody be demonstrated.

Immunologic Methods

Venous blood was obtained from each mother and refrigerated until the tests were performed, a period no longer than 72 hours.

Complete antibody† was detected by

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†Without any implication as to the structure of the antibody-globulin molecule the term "complete" antibody is used herein to indicate one which gives a visible agglutination in the usual system using saline as a diluent, as well as in albumin or serum. The term "incomplete" antibody is used to indicate one which gives a visible agglutination only when the water content of the system has been kept low by the use of serum or albumin as a diluent (Diamond and Denton[8]). This phenomenon has been associated with antibodies described under a variety of names—"conglutinating", "univalent", "blocking", "mature", "hyperimmune", etc. These two types of antibodies may or may not eventually prove to be separate and distinct.

the agglutination method of Levine and Stetson.[4] Incomplete antibody was detected by the "conglutination" test of Wiener,[7] the slide test of Diamond and Abelson,[2] and the anti-human serum technique of Coombs, Mourant, and Race.[1] The latter test has in our hands proved to be the most delicate method for proving the presence of weak antibodies.

The anti-Rh_0, anti-Rh′ and anti-Rh″ sera used in Rh typing were obtained from the Blood Grouping Laboratory, Boston.*

Results

A careful check of the type of feeblemindedness in each child revealed that one of the original group was a cretin. This patient was therefore eliminated from the present study. The mother of a second child was found on recheck to belong to blood type Rh_0 and was likewise eliminated. Of the remaining fourteen mothers, only eleven could be located.

The pertinent findings are shown in Table I.

The following histories were obtained in the two cases which were thought to be compatible with Rh immunization.

Case No. 7

This mother (Rh negative) had two uncomplicated pregnancies which resulted in normal children. There was no history of jaundice, anemia or symptoms relative to the central nervous system in either of these offspring. Their subsequent development was normal. While these children were quite young the father died. Several years later the mother remarried and had one child by her second husband. This baby (Rh positive) became severely jaundiced on the second day of life. He presented a definite feeding problem, and the mother states he "never did well." It soon became quite evident that the child was mentally impaired; however, he was not spastic and he never developed athetosis. He is now thirty years of age, is emotionally unstable and on occasions displays homicidal tendencies.

Case No. 9

This mother (Rh negative) delivered one normal child. Her second pregnancy four years later resulted in the birth of a baby girl who was jaundiced. This child had difficulty in feeding and failed to develop normally. At six months of age it became evident to the parents that the child was mentally deficient. She was taken to a nationally known clinic for study. The parents were told that she would never walk or talk. However, at the age of five years the child learned to walk. She is now 16 years of age and is able to utter a few distinguishable phrases. (This mother's serum was found to contain anti-Rh antibody.) She subsequently

TABLE I. Data from Histologic and Immunologic Studies on 11 Feebleminded Children and Their Rh— Mothers.

Mothers	Rh Type	Slide Test	Conglutination Test	Agglutination Test	Anti-human Serum Test	Maternal Transfusion	Pregnancy Resulting in Affected Child	Total No. of Pregnancies	Jaundice in Affected Child	Age of Child
Case 1	Neg.	Neg.	Neg.	Neg.	Neg.	None	2nd	5	—	14
Case 2	Neg.	Neg.	Neg.	Neg.	Neg.	None	1st	1	—	15
Case 3	Neg.	Neg.	Neg.	Neg.	Neg.	None	1st	1	+	15
Case 4	Neg.	Neg.	Neg.	Neg.	Neg.	None	2nd	3	—	12
Case 5	Neg.	Neg.	Neg.	Neg.	Neg.	None	6th	6	—	18
Case 6	Neg.	Neg.	Neg.	Neg.	Neg.	None	3rd	5	—	25
Case 7	Neg.	Neg.	Neg.	Neg.	Neg.	None	3rd	3	+	30
Case 8	Neg.	Neg.	Neg.	Neg.	Neg.	None	1st	6	—	38
Case 9	Neg.	+	+	Neg.	++++	None	2nd	3	+	15
Case 10	Neg.	Neg.	Neg.	Neg.	Neg.	None	11th	11	—	18
Case 11	Neg.	Neg.	Neg.	Neg.	Neg.	None	3rd	5	—	32

*These sera were supplied through the courtesy of Dr. L. K. Diamond.

had a miscarriage at three months' gestation.

Discussion

A relationship of Rh immunization to undifferentiated feeblemindedness has been postulated because of two observations. First, it is known that feeblemindedness, as well as convulsions, spasticity, and athetosis, is one of the concomitant results of kernicterus.[7] Almost invariably this pathologic condition is a manifestation of erythroblastosis fetalis, now known to be due to Rh incompatibility between mother and child. Secondly, Yannet and Lieberman,[10] and Snyder, Schonfeld, and Offerman[5] observed in studying 232 undifferentiated mental deficients that there were 35 Rh positive children of Rh negative mothers, whereas only 18.56 such mothers would have been expected on the basis of chance.

Clinical findings leading to the presumptive diagnosis of kernicterus in patients surviving erythroblastosis occur in less than ten per cent of cases.[7] Therefore, the implication that Rh immunization in the mother accounts for a large number of feebleminded children seems to have been overemphasized.

Since the observed frequency of Rh negative mothers in this study was twice that expected in the general population, and since data on 11 of the children were obtained, then six of these mothers might be assumed to be Rh negative and not immunized. The remaining five Rh negative mothers should represent those theoretically immunized by their Rh positive children. However, only two had histories even suggesting such immunization, and by laboratory methods only one was found to carry antibodies in her serum.

It must be noted that all of the children studied were over twelve years of age. It is known that the incomplete antibody, which is the antibody present in all mothers with erythroblastotic children, is quite stable and may be present many years after the last immunizing experience. Diamond[3] mentions one woman in whom such antibodies were still present in high titer 30 years after the birth of her last child. We have had the opportunity of demonstrating such antibodies 5 and 8 years later. On the other hand we have recently followed one patient whose antibodies declined and were absent at the end of one year after the delivery of an infant who had erythroblastosis. The lack of correlation between erythroblastotic complications and antibody titer, and persistence of demonstrable antibodies in the mother's blood, is one of the perplexing features of this problem. Hence, failure to demonstrate anti-Rh antibody suggests that the mother never was immunized against the Rh factor, but is not proof of this assumption.

The data obtained from the histories of these mothers provide additional evidence against the occurrence of maternal hypersensitivity. For example, in three instances the feebleminded child was the result of the first pregnancy. Erythroblastosis rarely affects the first-born child and then usually only in those instances where the mother has previously been transfused with Rh positive blood. Furthermore, erythroblastosis having occurred once in a family usually recurs in all the subsequent Rh positive offspring. Seven of the mothers delivered subsequent children after their feebleminded children. There were no abnormalities which could be attributed to the action of maternal antibody in the 21 subsequent children. According to our present knowledge the early miscarriage in case 9 cannot fairly be ascribed to this mechanism.

Only three of the 11 children restudied had histories of jaundice at birth. Jaundice is one of the salient features of erythroblastosis and is usually so marked that the mother would be likely to remember the fact.

We can conclude from the considerations mentioned above that case No. 9 represents the only instance which clinically could reasonably be considered to be due to Rh immunization of the mother. The present work suffers from being performed on too small a series of pa-

tients and after much too long an interval following birth. The findings offer no support to the theory of Yannet and Lieberman. Neither do they disprove it. However, the data on which the relationship was originally postulated are statistically significant. Acceptance of the theory requires immunologic confirmation. It is to be hoped that a large-scale experiment, embodying the criteria outlined in the introduction to this paper, will be undertaken in the near future.

Summary

1. The case material comprising Snyder, Schonfeld and Offerman's previous reports postulating a relationship between the Rh factor and undifferentiated feeblemindedness has been restudied from an immunologic point of view.

2. One instance of Rh sensitization has been found among 11 Rh negative mothers who delivered Rh positive children.

3. No instance of erythroblastosis fetalis was found among the subsequent children of any of these mothers.

4. These results offer no support for the theory that Rh incompatibility between mother and child is responsible for feeblemindedness in the offspring. On the other hand, the study was not performed soon enough following delivery nor on a large enough series to afford definite disproval of such a theory.

5. Criteria for an adequate experiment designed to prove or disprove this relationship have been set forth.

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Editorial Subconscious Slashes Prices: A Correction

A typographical error on page 160 of the May 1947 issue of the Journal of Heredity listed the price of *Personal Counsel* by Robert Frank (Informative Books, New York, 1946) as $.50. The correct price is $3.50.

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